LETTERS TO THE EDITOR

Induced abortion as an independent risk factor for breast cancer

Sir—In a recent paper Brind et al. investigated the relationship between abortion and breast cancer in a so-called meta-analysis. They used data from published reports to investigate whether induced abortion increases the risk of breast cancer. Their stated aim was to ascertain and quantify this risk. However, there are several methodological weaknesses in this analysis which bring into question their conclusions.

Firstly, the authors included only papers that have been published. It is well known and often discussed that publication bias poses a crucial problem for the interpretation of results from meta-analysis. Papers reporting a positive effect are more readily published than papers showing no effect. Additionally, in most epidemiological studies, data are collected on many variables but they are only published in a selective way. Data on suspected risk factors tend to be published if results are striking. The authors briefly discuss this problem, but they argue that due to the “politically and legally sensitive” topic, publication is not biased towards a positive association with breast cancer incidence—rather towards a negative one! This argument is untenable. It seems to be motivated by a strong belief held by the authors but not by any scientific reasoning.

Secondly, the authors performed a literature search with Medline. It is well known that this is not even sufficient to obtain all published papers. Further work would be necessary for a comprehensive review—e.g. to search the references of published papers, to contact researchers, and to search through proceedings and abstracts of meetings. The authors used three specific key terms only; namely “abortion”, “breast”, and “cancer” in their Medline search. Papers with negative results on abortion might not use the term “abortion” in the title or in the abstract if the results were not the major findings. Other papers might use other terms such as neoplasms, pregnancy, mammary carcinoma. These articles would not have been located by their procedure. The literature search would thus have been strongly biased towards papers that mentioned abortion in the abstract, in effect identifying mainly papers where abortion had a “significant” effect.

Apart from English papers, the authors included Japanese, Portuguese, and Russian papers. No justification for this specific selection was made. Were there no publications in French, German, or Chinese?

The inclusion criteria were not clearly laid out. Although the authors stated in the text that publications after 1966 were included, both tables (1 and 2) also included papers that were published in 1960 and 1957, suggesting possible selection by the authors. Selection bias was most apparent with regards to the first reference, a study that should not have been included because it appeared in 1957 before the time period being considered. In addition, odds ratios were not given in the report of the study and the authors went out of their way to perform a quite complicated calculation to estimate an odds ratio. The inclusion of this study influenced the results towards achieving a positive association.

To quantify the risk of breast cancer associated with induced abortion, the authors calculated a weighted average and an unweighted average of the odds ratios. No investigation of heterogeneity was performed before combining the data. In case of heterogeneity, a pooled estimator should not be calculated. We performed a test of homogeneity and found that the heterogeneity between studies was highly significant ($p<0.001$). This is a contraindication for estimating a pooled odds ratio (see fig 1, radial plot). Six out of 21 studies gave an odds ratio outside the prediction limits around the pooled estimate. Two out of three “positive” outlier studies were from Japan, while the larger and more recent studies (references 67 and 61) showed estimates far below the pooled odds ratio.

Odds ratios included in the calculation of a “pooled estimate” were derived in very different ways. Five studies reported only raw (crude) odds ratios. In several other studies, however, odds ratios were calculated using statistical regression models, adjusted for different confounding factors in multivariate analysis. The combination of crude and model based odds ratios may yield biased results. In addition to this, the reference group (women with no pregnancy or women with any pregnancy) was not consistent in all studies.

Differences between the studies with regard to study design, data collection, and statistical modelling were not presented in a systematic way. The possible influence of the choice of controls and of the method of data collection (questionnaire, interviews) was not discussed. It would have been instructive to perform the analysis separately for the different types of studies.

The description of the individual studies is in part overly extensive, but not aimed at bringing out the differences. After reading the introduction and the discussion we gained the impression that the authors had a very distinct opinion about the subject. All the papers in which positive results were obtained and cautiously interpreted by the communicating authors, were negatively reviewed by Brind et al.

The results of this meta-analysis seem to be strongly biased towards a positive effect—mainly due to the way studies were selected. Standard statistical methods that are available for meta-analysis (see, for example, 1), in particular for the investigation of heterogeneity, were not used. We feel that it is unethical and harmful to publish such data as definitive results or facts of “meta-analysis” from observational studies when only a pooled estimate (with 95% confidence interval) is calculated, without investigating the heterogeneity between studies. Results from meta-analysis are often used and cited for further research and for implementing public health policies. In general, meta-analysis using only published data from epidemiological (observational) studies and not the original data set should be treated with caution. Some authors even argue that such a meta-analysis cannot yield reliable results in the presence of heterogeneity.

The conclusions drawn in the work of Brind et al are not based on a methodologically sound investigation and are therefore not justified.

MARIJA BLETTNER
JENNY CHANG-CLAUDE
THOMAS SCHEUCHENPFULL
German Cancer Research Center,
Division of Epidemiology, 69120 Heidelberg,
Germany


Reply

We welcome substantive criticism of our "Comprehensive review and meta-analysis" and the opportunity to respond to Blettner et al, although we vigorously dispute their objections.

First, they largely conclude that our results appear “strongly biased towards a positive effect—mainly due to the way studies are selected for the meta-analysis.” To support this contention of “selection bias,” they cite inclusion criteria that are simply not to be found in our paper. Nowhere did we limit our inclusion to papers published only since 1966, which is the earliest year that the Med-
Further evidence of a lack of bias against publication of negative data is provided by the recent literature concerning oral contraceptive use and breast cancer, a literature which overlaps considerably with that con

cerning induced abortion and breast cancer. In designing their 1990 “review and meta-

analysis” on oral contraceptives and breast cancer, Romieu et al employed reasoning no different from ours, but without the simple expedient of pooling results from all available studies. They also argued against the existence of significant publication bias against null results, reasoning that the absence of such bias would stand out clearly in relative risk versus study size, and concluded that, “There was no pattern, sug-

gesting the absence of systematic bias in publication”. (The lack of such a trend among the studies included in our own meta-analysis is also clear.) What better proof of the validity of this reasoning and this conclusion, than for the massive “collaborative reanalysis” published last year in The Lancet, which in-

cluded unpublished as well as published data, to arrive at the same overall result as had Romieu et al (overall relative risk: 1.07 ± 0.017 versus 1.06 ± 0.08, respect-

ively)?

The third criticism listed by Blettner et al relates to heterogeneity. That is, they fault us for performing “no investigation of hetero-

genesis”. Indeed, we performed no quanti-

tative test for heterogeneity in our studies, for lack of desire to prove the obvious, having duly noted that the included studies “differ widely in size and in many aspects of study design,” but nonetheless display “a remarkably consistent, significant positive as-

sociation between induced abortion and breast cancer incidence”. We noted this con-

sistency of a positive trend in our table 4, which showed that 16 of 21 studies (76%) reported an overall positive association, 10 with statistical significance. Here again, more recent reports have confirmed this trend, with 22 of 28 studies extent as of this writing reporting a positive association, 17 with stat-

istical significance.

We also duly note the quantitative hetero-

geneity pointed out by Blettner et al, but disagree that this constitutes “a con-

tradiction with the pooled odds ratio”. It may be argued, however, that a random effects model would have been more appropriate for arriving at a pooled odds ratio.

We chose a fixed effects model in order to obtain the estimate of the pooled odds ratio (1.3; 95%CI 1.2, 1.4). A random effects model would be expected to yield an estimate closer to the unweighted average. We also calculated and presented the pooled, unweighted average (1.4; 95%CI 1.3, 1.6) which was in good agreement with the weighted average. We have now calculated the pooled weighted average of the 21 studies in the meta-analysis following a random model, and it is the same as our initial result (1.3, 95%CI 1.1, 1.6), albeit with a wider confidence interval. Importantly, inclusion of all studies to date into this calculation (n =

28) yields this new result, the more recent data serving only to narrow the confidence interval (1.3, 95%CI 1.2, 1.5), thus con-

firming our initial result.

Yet Blettner et al carry the heterogeneity criticism even further, claiming: “The com-

bination of crude (5 studies) and (logistic regression) model based odds ratios (16 stud-

ies) may yield biased results”. One wonders where these criticisms come from our extensive discussion of just this point. In fact, we re-

culated the weighted odds ratio and confi-

dence interval, based on 15 of the 16 studies which used multivariate models, omitting the one which did not account for age at first full-term pregnancy. This choice appears to be one of the “positive out-

lying studies” in the radial plot prepared by Blettner et al. The weighted average thus obtained for these 15 studies did not differ significantly from the overall weighted average of all 21 studies, and it was still sig-

nificantly positive (1.2; 95%CI 1.1, 1.3).

Finally, although it is our preference to focus solely on scientific criteria, they also believe it is appropriate to discuss the “politically and legally sensitive” nature of studies suggesting harmful effects of induced abortion, par-

icularly in light of our critics taking such excep-

tion to our acknowledgement of that fact. Indeed, the tone of hostility in the sug-

gestion of Blettner et al that the publication of our “so-called meta-analysis” was “unethical and harmful” is inescapable. Our study docu-

mented a literature in which the over-

whelming majority of studies (16 of 21) had reported a positive association between in-

duced abortion and breast cancer incidence, a fact entirely consistent with the results of Blettner et al. We are thus speaking of results such as ours to be treated with caution”. But caution in regard to what? Or to whom? Are they not aware that induced abortion is overwhelmingly the least harmful of all procedures? Would not caution in regard to proper patient care demand acknowledgement of the pre-

ponderance of available evidence that abor-

tion is not only a risk factor, but the most avoidable of risk factors for breast cancer? Indeed, what perversity of standards of in-

formed consent and patient care could place ethics and harmlessness on the side of with-

holding such medical care from women who must live (or die) with the choices they make regarding abortion? That the criticisms raised by Blettner et al against our review and meta-analysis do not stand to reason is merely noteworthy, but the nature of the bias revealed in their critique (and in this they are far from unique) is deeply disturbing.

JOEL BRIND

Department of Natural Sciences, The City University of New York, New York, NY 10013 USA

VERNON M. CHINCHILLI

WALTER B SEVERS

JOAN B. CHINCHILLI

LONG CENTER FOR BIOSTATISTICS AND EPIDEMIOLOGY

AND DEPARTMENT OF PHARMACOLOGY,

PENNSYLVANIA STATE UNIVERSITY COLLEGE OF MEDICINE

Hershey, PA 17033 USA

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traceptives and breast cancer: collaborative reanalysis of in-
NOTICES


Guest editorial
Citizens, users and health care M Calnan

Editorial note
Editorial note on journal development. P-G Svensson, J Palm

Original articles
Assessing the value of accreditation systems. B Screens
The relation between the number of symptoms and other health indicators in working men and women. I Popa, RH Noack
Suicide reporting in Swiss print media: responsible or irresponsible? C Frey, K Michel, L Vidal
Predictors of disability pension among young men: the role of alcohol and psychosocial factors. M Umapark, T Hemmingsen, A Romedjo, T Lundberg, P Allbeck
Oral contraception and smoking: time trends for a risk behaviour in Finland. E-A L Kosunen, AH Rimpela, JKA Kaprio, M-A Berg
Appropriateness of hospital use: report from an Italian study. G Apelone, G Felto, A Tampieri, E Bonanomi, PF Crosti, E Lansi, G Menghelli, G Trocino, A Liberati
Income distribution and cause-specific mortality. SJ McIsaac, RG Wilkinson
Effects of primary health care reform on the prescription of antibiotics: a longitudinal study in a Spanish county. S Juncosa, M Porta
Hospital strategy reform in Romania: an experiment in Bucharest in 1993. A-L Le Faou, A Marlier-Sutter, D Jolly

Mammographic screening
Economic evaluation of a mammography-based breast cancer screening programme in Spain. R Garus, T Forcen, J Cabadas, F Antohanzai, C Trinxer, J Rovira, F Avion
How susceptible is the answer? Female population to mammographic screening? GF Van Hal, TJ Wyler, MM Matthysen

Perinatal and child health
Ethnic differences in rates of infant mortality and sudden infant death syndrome in

Sweden, 1978 to 1990. CEM Oldenburg, F Rasmussen, NU Cotten
Childhood immunization: meeting targets yet respecting consent. P Alderson, B Mayall, S Barke, J Henderson, B Pratten

Short reports
A graphical display useful for meta-analysis. PJ Jimenez, E Guadal JM Martin-Moreno
Association of waist to hip ratio with diabetes among middle-aged subjects with impaired glucose tolerance. Q Qiao, S Keißen-Kiukasniemi, S-L Kitoku
A population-based study of familial clustering of inflammatory bowel disease in Florence. D Palli, C Saieva, G Trallori, G Bardassi, G d’Albasio, M Milita, M Galli, O Taraninto, F Pacini
Letter to the editor
News
Book reviews
Calendar of events
EUPHA section

Epidemiology—past and future. A joint meeting between the UK Society for Social Medicine and the Journal of Epidemiology and Community Health is to be held on Thursday, October 30, 1997 at the Weston Theatre, Manchester Conference Centre, UMIST from 10.00—16.30. Further details from Professor Charles Florey, Dept of Epidemiology and Community Health, Ninewells Hospitals and Medical School, Dundee DD1 9SY. Tel 01 382 632124. Fax 01 382 644197


BOOK REVIEWS
Health planning consists of formulating health goals and the means necessary for achieving these. Planning is thus a rational way of allocating resources so that the future can be steered in the most desirable direction. Setting objectives involves projecting foreseeable health scenarios, contrasting situations where healthcare intervention is absent and then where it is present. It is, therefore, extremely useful to have a software package capable of formulating future scenarios linked to the frequency of cardiovascular disease, and exploiting the consequences thereof on the health of the general population, the public health system, and, in particular, the distribution of any related costs.

The software package, designed for DOS-based computers, comes complete with a user’s manual which includes full information on the cardiovascular disease model used, development of the financial aspects, plus a wide ranging review of current knowledge on risk factors for cardiovascular disease and the efficacy of available preventive measures and methods of treatment.

The software is user friendly. Construction of scenarios and evaluation of the ensuing consequences requires data on cardiovascular disease risk factor distribution in the target population; the expected effect of healthcare intervention on this distribution; cardiovascular disease incidence and fatality; and the natural history of the disease. The absence of intervention. The fact that experts from the Mathematics, Health Economics, and Cardiology Departments at Limburg University, collaborated on this project has ensured that both software and manual are marked by methodological rigour. In all three disciplines, book and diskette will prove a useful tool to those interested in health planning and a source of inspiration for the development of new products in this field.

Lastly, though software and manual are of universal interest, the fact that they are specifically based on the Dutch population and related cardiovascular disease risk distribution detracts somewhat from the program’s utility vis-à-vis other populations. Furthermore, it would have lent greater insight, had the manual contained some information on the experience and impressions of Holland’s Public Health Authority planners on using this software in the course of their routine work.
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M Blettner, J Chang-Claude and T Scheuchenpflug

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